

## ***Interactive comment on “Amelioration of marine environments at the Smithian–Spathian boundary, Early Triassic” by L. Zhang et al.***

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1) This is an interesting and well-written manuscript that provides further evidence for environmental changes related to postulated cooling at the Smithian–Spathian transition after the Early Triassic hothouse. This work is a valuable contribution applying various geochemical proxies to argue for changes in the weathering rate as well as the oceanic circulation. These environmental changes are further discussed in the light of concomitant changes in the faunal and floral associations.

Response: We thank the reviewer for these comments.

There are some critical points that should be addressed before this ms can be accepted for publication:

C7869

2) 15362, line 10 to 11: .....d13Ccarb(+4 permil) and d34SCAS (-14 permil)..... Please explain what is meant by +4 permil or -14 permil – maximum values; shift in isotope ratios or amplitude of an excursion...?

Response: This refers to the concurrent ranges of variation shown by d13Ccarb and d34SCAS at Shitouzhai during the SSB event.

Action: We have reworded this sentence to more clearly express our intended meaning.

3) 15362, line 23 to 24: no new data for a stewise recovery are presented in this contribution .. why then mentioning this in the abstract?

Response: Agreed.

Action: We have deleted this sentence in the abstract.

4) 15365, line 2-3: the authors argue that the carbon isotope signals are not significantly affected by diagenetic alteration. However, the line of argumentation in Appendix B is not convincing and unclear. A special focus is on the correlations between the isotope ratios and Mn and/or Sr contents. If the authors want to use manganese contents as an indicator for carbonate diagenesis, only Mn<sup>2+</sup> substituting Ca<sup>2+</sup> in the calcite lattice should be considered (same for Sr). However, no information is giving on the methodology of dissolving the rocks and whether the trace element contents refer to whole rock or represent the carbonate-bound fraction. If the given Mn and Sr contents refer to whole rock Mn, no interpretation with respect to a potential diagenetic alteration of the carbonates is possible, since the trace element concentration will not exclusively correspond to Mn and Sr in the calcite lattice.

Response: The reviewer raises a good point. The Mn/Sr ratios cited in the text are for whole-rock samples. However, the study samples are mostly carbonate, containing an average of just  $1.10 \pm 0.60$  percent Al (a proxy for clay-mineral content). We tested to see whether accounting for Mn and Sr in the clay-mineral fraction would have any effect on Mn/Sr ratios as follows: (1) we estimated the detrital fraction of Mn and Sr

C7870

in each sample based on the Al concentration of each sample and Al, Mn, and Sr concentrations of 8.04 percent, 600 ppm, and 350 ppm, respectively, for average upper continental crust (McLennan, 2001); (2) we subtracted these values from whole-rock Mn and Sr concentrations to arrive at estimated carbonate Mn and Sr concentrations; and (3) we calculated carbonate Mn/Sr ratios based on these estimates. The carbonate Mn/Sr ratios are nearly identical to the whole-rock Mn/Sr ratios: the distribution of the former (as given by 16th-50th-84th percentile values) is 0.10-0.35-2.76 and that of the latter is 0.16-0.39-2.70. The two distributions of Mn/Sr ratios are very similar because detrital Mn comprises just  $8.1 \pm 4.4$  percent of whole-rock Mn, and detrital Sr comprises just  $4.0 \pm 2.2$  percent of whole-rock Sr. We could introduce these calculations into the manuscript, but it would add unnecessary complexity.

Action: We have added a statement to Appendix B that calculated carbonate Mn/Sr ratios are nearly the same as whole-rock Mn/Sr ratios.

5) 15365, line 22-23: limited fossil occurrences allow recognition of three conodont zones. The authors illustrate these conodont zones by differently coloured bars in Figure 1. However these zones are defined only by 4 determinations of conodonts (blue dots in Figure 1), which is absolutely inadequate to define the illustrated conodont zones. I recommend to delete the coloured bars in Figure 1 and instead correlate the sections exclusively based on carbon isotope patterns, the latter being convincing.

Response: The conodont zonation for the West Pingdingshan reference section in Figure 1 is well established (Zhao et al., 2007). The conodont zonation that is shown for Shitouzhai is not based on the limited ( $n = 4$ ) conodont identifications of the present study. Rather, it is a “model” zonation scheme based on the detailed C-isotope correlations shown in Figure 2.

Action: We have added the citation to Zhao et al. (2007) and have clarified these points in the caption of Figure 1.

6) 15366, line 10: Triassic conodont samples of 3 to 4 kgs are not really large.

C7871

Response: This study is primarily a geochemical study, and the Shitouzhai section was sampled for that purpose, not for conodont extraction. The collected material was more than adequate for the geochemical analyses that were undertaken.

Action: None requested by reviewer.

7) 15366, line 21: the authors have to state whether the given stddevs are 1 or 2 sigma. In addition, they have to provide information how the analyses were calibrated to the V-PDB scale. Measuring a laboratory standard is not sufficient.

Response: The reported standard deviations are 1 sigma. V-PDB is short for Vienna Pee Dee Belemnite with  $\delta^{13}\text{C}_{\text{carb}}$  value given as 0‰. The  $\delta^{13}\text{C}_{\text{carb}}$  values of our samples are reported relative to this standard, not the calibration. GBW-04416 is our national standard reference material and used as our laboratory standard.

Action: We have added a note regarding this to the Methodology section and have changed “laboratory standards” to “national reference standard”.

8) 15367 line 22: please give details on how the analytical uncertainty was determined.

Response: Multiple measurements of the international standards BHVO-2 and BCR-2 were employed to calculate analytical precision. Analytical precision of the measured samples is mostly better than 2

Action: We are changing the method used to report analytical uncertainty, basing it on RSD (which is generally to be preferred) rather than on absolute concentrations. We have modified this part of the Methods section accordingly.

9) 15368 line 18: the minimum N4 and the correlation of N4 in various sections (Figure 2) is rather a guess and not really convincing. I recommend to omit the N4 correlation from Figure 2.

Response: There is a clear minimum in carbonate C-isotope curves during the mid-Spathian (e.g., Payne et al., 2004; Tong et al., 2007). Song et al. (2013) labeled

C7872

this minimum N4. For all C-isotope excursions of the Early Triassic, some appear as sharp peaks in some sections, and as broad excursions in others. Although the broad excursions may not permit as exact a correlation as the sharp peaks, all are useful for global correlation of C-isotope curves. The mid-Spathian minima shown in Figure 2 clearly represent N4, even where they appear as a broad excursion (as in the Majiashan section).

Action: No change.

10) 15369, line 2: How can the  $\delta^{13}\text{C}_{\text{carb}}$  and  $\delta^{34}\text{S}_{\text{CAS}}$  values have a significant negative correlation in case a positive (?)  $r^2 = +0.14$  is calculated. This statement has to be revised and the significance of all correlations has to be underlined by further statistic tests

Response: The reviewer's comment reflects a lack of understanding of basic statistics. One must recognize the difference between (1)  $r^2$ , which is the "coefficient of determination" and represents the covariance of two variables; this statistic ranges from 0 to 1.0; and (2)  $r$ , which is the "correlation coefficient" and represents the linear relationship of two variables; this statistic ranges from -1.0 to +1.0. The two statistics are related to each other in that  $r^2$  is the square of  $r$ ; the square of any negative number is going to be a positive number.

Action: We converted the reported  $r^2$  value to an  $r$  value so that other readers do not make the same misinterpretation.

11) 15369 Chapter Sediment fluxes: it is completely unclear how the carbonate and clay mass accumulations were calculated. How were carbonate and clay percentages measured? No information is provided.

Response: The methodology of calculating sediment fluxes is described concisely in the caption of Figure 3. More detailed algorithms were presented in Algeo and Twitchett (2010), which is cited in the same caption. There is no need to repeat this material from

C7873

that earlier publication, to which the reader can refer if interested.

Action: None.

12) 15371 line 20: clay mineral production – please rephrase

Response: There is nothing wrong with this phrase. "Clay mineral production" refers to a combination of processes that generate clay minerals through weathering, including chemical breakdown of feldspars and other minerals, and recycling of clay minerals from older shales and mudstones.

Action: None.

13) 15372 line 4: no Th-normalized element ratios are provided in the appendix...

Response: This is an oversight on our part.

Action: We have added Th-normalized concentrations for the cited elements to Table C2.

14) Appendix 2:  $\delta^{18}\text{O}_{\text{carb}}$  and  $\delta^{13}\text{C}_{\text{carb}}$  values show an  $r^2 = 0.0001$  which means that the two variables are not correlated. The authors state a "weak covariation"? Please rephrase throughout Appendix 2.

Response: We intended the phrases "weak covariation" and "little covariation" to mean "no significant covariation", but we agree with the reviewer's comment.

Action: We have reworded all such statements to make clear that there is no significant covariation.

15) Tables C1 and C2 of the supplement correspond to Figs A1 and A2 of the Appendix. Why this?

Response: We apologize for any confusion in the labeling of tables and figures in the Appendix.

Action: All tables and figures are correctly labeled in the revised manuscript.

C7874

16) Fig. B1a: why presenting a plot of d18O vs. Mn/Sr. Oxygen isotopes are not discussed in this paper

Response: The relationship between d18O vs. Mn/Sr is discussed in Appendix B, as are all geochemical relationships that were used to evaluate the diagenesis of the study section. The detailed discussion of diagenesis was put in an appendix rather than in the main text because it is not essential to the development of the main theme of the paper.

Action: None.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/11/C7869/2015/bgd-11-C7869-2015-supplement.pdf>

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